An exposure of the misrepresentations in the Philosophical Magazine and Annals, for December, 1830, in its attack upon the author's Elements of practical chemistry / by David Boswell Reid.

AN EXPOSURE

OF THE

MISREPRESENTATIONS

IN THE

PHILOSOPHICAL MAGAZINE AND ANNALS,

FOR DECEMBER, 1830,

IN ITS ATTACK UPON THE AUTHOR'S

ELEMENTS OF PRACTICAL CHEMISTRY,

BY

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> In trusting to the sentence of a critic, we are in danger from that vanity which exalts writers too often to the dignity of teaching what they have yet to learn.—JOHNSON.

MACLACHLAN AND STEWART, EDINBURGH.

JANUARY, M.DCCC.XXXI.

This Answer would have appeared earlier, had not my different engagements occupied me so completely, that it was with difficulty I found time t attend to it at all.

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EXPOSURE OF THE MISREPRESENTATIONS, &c.

I AM so fully aware of the awkwardness of that excessive sensibility which an author is apt to betray when animadversions are made on his work, and of the propriety, in all ordinary cases, of leaving matters to the decision of the public, that I should certainly never have obtruded myself on their notice, merely because I had received a share of that rough handling to which far abler men must submit, and which, I have not the presumption to think, my book may not deserve. This course I should the more readily have adopted from the indulgent reception which my Elements of Practical Chemistry has met from several journals of the highest respectability, a reception no doubt partly owing to their unwillingness to discourage a young author in a new and rather arduous attempt to systematize and facilitate the study of Practical Chemistry.

But the extraordinary character of this review, the grossness of its misrepresentations, and the unaccountable ignorance of this personage who comes forward as a public censor, I feel myself called upon to expose, otherwise the hardihood of his assertions might stand for proof with those who have merely a general knowledge of chemistry.

In this review there is not only an attempt to "run down" my book, but the writer lets his zeal so far outrun his wit as to travel out of the record—and he volunteers his services to injure my character as a teacher and professional chemist. Had the reviewer confined himself to general expressions, whatever might have been the spirit in which they were conceived, this appeal would be unnecessary; his mere authority might be safely left to find its own level, as the trade of "cutting up," and the various motives which lead to it, are now pretty well understood by the public. It is the price we must pay for the benefit of free discussion, and for the advantages we derive from those superior minds, whose candour, discernment, and learning, have given such value to our periodical literature.

But this writer has, with an infatuation for which it is not easy to account, condescended to be specific in his charges, and has thus exposed himself in a way that enables me to bring matters to a direct issue, without the possibility of his escaping in loose generalities. For it must be borne in mind that his charges do not refer to mere opinions and speculations, but to matters of fact, where an appeal to experiment, easy of execution, and involving no doubtful position, can at once afford the most unequivocal evidence of the accuracy or inaccuracy of the statement made.

Nor does he confine his attack to me. In the very first sentence he steps out of his way to implicate a distinguished professor, and has the presumption to insinuate that he must have superintended the work, because I happen to be his experimental assistant.

Quotation I. "This work is presented to the chemical student under circumstances calculated to raise high expectations of its value. Mr. Reid was formerly the pupil, and is now the chemical assistant of Dr. Hope,—a lecturer who is justly celebrated for the excellence and accuracy of his experimental illustrations; to him the book is dedicated, and we may, perhaps, not unfairly presume that it did not make its appearance entirely without his sanction." p. 449.

The impropriety of this charge, and the alacrity which it shows in this critic to labour in his vocation, need no comment. I have only to observe, that the insinuation is without the least foundation.

The Practical Courses, which I had established on a new system of teaching before my connexion with Dr. Hope, have nothing to do with my duties as experimental assistant, further than the liberty I have of giving them within the University; though I can now state that, after an examination of my plan and arrangements for conducting these courses, he has been pleased to express his approbation of them. The leading feature of this system is, that while the student is enabled to operate every day, the range of processes and experiments which it includes, and its general efficiency as a branch of chemical education, are extended to a degree that had not before been contemplated.

As to my work, whatever may be its defects, I alone am responsible for them, as its plan and execution are entirely my own, nor did Dr. Hope even see it till it was printed: it had not the benefit of so valuable a revision.

This reviewer proceeds to state—

"*Quotation II.*—"Thus advantageously situated, Mr. Reid must excuse us, if we examine his pretensions, which are by no means slight, with some degree of minuteness."—

"We shall not pay any attention to Mr. Reid's arrangement, nor proceed regularly through the work, but confine our remarks principally to the chapter on Nitric Acid." p. 449.

This is rather a serious note of preparation. It pretty clearly indicates the intention to be very severe. It promises a terrible "cutting up," and therefore some amusement to those who enjoy the sport of seeing an author "baited."

This doughty critic, who places himself as a judge at the gate of science, feels, no doubt, that he is called upon, in the exercise of his high function, to do violence to the native delicacy of his mind, and discharge a painful public duty.

"———He cannot flatter, he!—
An honest mind and plain,—he must speak truth:"

He proposes to make me an example to evil doers. With this view he is to concentrate all his forces on one point, and has selected the operations connected with nitric acid for the fury of his onset; partly, no doubt, because he thinks me particularly vulnerable there, and partly because he is quite satisfied of his own strength on that point. Whether, in thus confining his attention to a single chapter, and passing over the claims of a work which professes novelty of plan, arrangement, and illustrations, which embraces a wide and comprehensive system of operations, where the mass of matter could not be restated from former authorities, but required precise observation and personal experience, and which therefore had peculiar difficulties to encounter,—whether, I say, passing over all these is altogether fair, I do not pretend to determine. I can only state that five other reviews in this country have noticed my work, and that each has brought these points prominently forward. But I am most willing to meet this worthy reviewer on his own ground, and likewise to let my book stand or fall according as I can prove his ignorance, his misrepresentations, and his clumsy and ill-disguised efforts to depreciate my work. To prevent all ambiguity, and that the reader may be put in full possession of the subject, I shall quote the reviewer's own words in my reply.

That we may see what degree of confidence can be placed in the statements or experiments of this critic who professes a minute examination, it may be well to commence by examining an experiment performed by him for the express purpose of ascertaining the accuracy of a statement of mine.

> *Quotation III.*—" After stating that the nitric acid and water are both decomposed, he" (Mr. Reid) " says : ' when this takes place, the hydrogen of the water unites with the nitrogen of the acid, forming ammonia, which combines with part of the nitric acid that is not decomposed, forming nitrate of ammonia : this explains,' continues Mr. Reid, ' the appearance of the white fumes which are often seen intimately blended with the nitrous acid vapours that are formed when this acid is decomposed by a metal having a great affinity for oxygen.' "
>
> " For our part, we must confess that we had never observed this blending of white and red vapours during the solution of a metal; but *fiat experimentum* is our motto, and we accordingly mixed some diluted nitric acid and tin ; and we do admit that for a moment, and by the action of the heat generated before any considerable evolution of nitric oxide occurred, that some white vapours were visible ; they were, however, instantaneously succeeded by red fumes, in which it required powers of vision superior to those of second sight, to detect any blending of white vapour." Pp. 453-4.

In the two last lines of the second paragraph just quoted he tells us that he could not detect any white vapour mixed with the nitrous acid fumes after the latter had appeared in considerable quantity, as if thereby confuting a statement of mine, while in my words(which he himself has actually quoted in the preceding paragraph) there is no mention whatever made of any particular period at which the white vapours appear. This statement of his, that the white fumes are not visible during the latter part of the experiment, and which he brings forward with an air of authority and as a matter of the utmost importance, has nothing whatever to do with the point in question; while at the same time, he admits as the result of his experiment the appearance of the white vapours that I have described. My words are " white fumes are often seen intimately blended with the nitrous acid vapours." Thus, although he intends to prove that my account is incorrect, the only difference between us is, that I say " often," because I have seen this appearance often ; while he has only seen it once—that is, after he had seen it stated in my book, and in the first experiment he performed to ascertain the correctness of my statement, with the first metal that occurred to him, and the first kind of acid he employed.

This minute examiner tells us that *fiat experimentum* is his motto ; he performs an experiment to ascertain the accuracy of my description ; he sees what I have described ; at this stage of the experiment he forgets his motto, *and does not examine the vapours the composition of which is the point in question.* He has thus also distorted my statement in two ways, by making it appear, first, that I had said that the white vapours continued for some time, and next that I had described them as seen at the latter part of the process. This misrepresentation and the peculiar mode of experimenting to determine what he considered a doubtful point, are followed in the next sentence by an example of his information on this subject and his mode of reasoning, which are no less remarkable.

> *Quotation III. continued*—" We suppose that Mr. Reid takes the white vapour for volatilized nitrate of ammonia : but he must surely be aware that this salt is decomposed at a low temperature and not vaporized." p. 454.

It must appear not a little singular, that a statement displaying such ignorance of facts, and of works intimately connected with an important part of the history of the science, could gain admission into a journal which had a character to support. *It is not true that nitrate of ammonia, as he states, " is decomposed at a low temperature, and not vaporized." It is vaporized without being decomposed at a comparatively low temperature, and it is not decomposed till the* *temperature is increased to above that point at which it may be volatilized.* Did this reviewer ever hear of a gas called nitrous oxide? Did he ever prepare it? Did he ever read or hear how it is prepared? Does he not know that this gas is procured from the nitrate of ammonia by exposing the salt to heat—that the nitrate is then resolved into nitrous oxide and water, and that it is necessary to apply a considerable degree of heat, otherwise the salt is vaporized without being decomposed? The preparation of this gas is an experiment which I have performed repeatedly, which my pupils perform; and is well known to all chemists. I have frequently seen the nitrate vaporized without decomposition, and have taken out a stick of solid nitrate of ammonia from the beak of a retort in which it had been condensed during the preparation of nitrous oxide, and shown it to the class. If this writer, who undertakes to review works on practical chemistry, had not observed this in the course of his own experience, he might have read an account of it in Sir Humphrey Davy's Researches on Nitrous Oxide, the work which first brought that illustrious chemist into general notice, which excited so much interest among all classes by drawing the attention of the public to that singular gas the nitrous oxide, one of those gaseous combinations, with whose nature and properties people in general are perhaps better acquainted than with those of any other. Indeed, Sir H. Davy has, in the work alluded to above, a section devoted solely to the consideration of the temperature at which the nitrate of ammonia is vaporized and decomposed: from this part of the work I have selected the following passages:

"From a number of experiments made in this manner on different salts, the following conclusions were drawn.

"1st, Compact, or dry nitrate of ammoniac, undergoes little or no change at temperatures below 260°.

"2dly, At temperatures between 275° and 300°, it slowly sublimes, without decomposition, or without becoming fluid.

"3dly, At 320° it becomes fluid, decomposes, and still slowly sublimes; it neither assuming, or continuing in, the fluid state, without decomposition.

"4thly, At temperatures between 340° and 480°, it decomposes rapidly.

"5thly, The prismatic and fibrous nitrates of ammoniac become fluid at temperatures below 300°, and undergo ebullition at temperatures between 360° and 400°, without decomposition.

6thly, They are capable of being heated to 430° without decomposition, or sublimation, till a certain quantity of their water is evaporated.

"7thly, At temperatures above 450° they undergo decomposition, without previously losing their water of crystallization." *Davy's Researches*, pp. 85, 86.

These statements are very clear and explicit; the facts stated are now well known; yet the reviewer is surprised that I should think it possible that this salt could exist in a state of vapour and not be decomposed at a comparatively low temperature. He brings as an argument against these vapours being nitrate of ammonia, an assertion which is quite at variance with the fact. What then can we think of a writer who, in reviewing a work on chemistry, displays such ignorance of well known facts established thirty years ago, and of a work with which it is inexcusable for a reviewer criticising this very subject to be unacquainted. His ignorance on this point can only be equalled by his singular mode of reasoning in the next part of the sentence.

Quotation III. continued.—"And in proof that it is neither decomposed nor volatilized in the present instance, it is well known that when lime is added to the oxide of tin, the nitrate of ammonia diffused through it is decomposed and the alkali evolved."

Any person not acquainted with chemistry might perhaps suppose from this statement, that if one portion of nitrate of ammonia is disengaged, the rest ought necessarily to accompany it, or that there was a chemical reason why some nitrate of ammonia remaining mixed with the oxidated metal was a demonstrative proof that none had been converted into vapour. But every tyro knows that this is not the case. Does this erudite gentleman not know that it is easier to expel a part than the whole? Has he never seen water slowly evaporated from one *salt*, and another *salt* slowly evaporated from water? and is he not aware that when evaporation is commenced by heat, it requires a continuance of the heat to complete it. But really this does not admit of a serious refutation.

It may be imagined, after the perusal of the preceding paragraphs, that nothing could be brought forward to show more clearly the spirit with which the review is written, and the utter incapacity of the reviewer for the task he has undertaken. But the reader will be still more surprised when he examines the writer's next paragraph, which is full of misrepresentations and inaccurate statements, and on a subject too with which he is quite familiar, in his own opinion; as he tells us at the outset, that it involves " no peculiar difficulty, either theoretical or practical."

Quotation IV. " Mr. Reid next notices the action of nitric oxide upon nitric acid, and the changes of colour which result from it. 'If a current of nitric oxide gas be transmitted through colourless nitric acid, a large quantity of this gas is absorbed, and the acid speedily acquires a light straw colour, which deepens to a reddish brown, and passes through various shades of olive and green till it at last becomes almost blue.' From this statement it would appear, that the only circumstance which determines the colour of the acid is the quantity of the nitric oxide which it absorbs. We have already been instructed to consider, that by nitric acid we are to understand that which is concentrated, and such we presume is that to be employed in this experiment; and if this be the case, the effects produced are described with extreme and most culpable inaccuracy. In the first place, strong nitric acid never becomes at all either olive, green, or blue by absorbing nitric oxide; and what proves that Mr. Reid never performed the experiment is, that when the colours are produced they do not occur in the order stated by him." p. 454.

In reply to this extraordinary assertion, I have to mention in the first place, that I have repeatedly performed the experiment; that it is not a difficult one; that the result may be obtained in the course of a minute or two; and that I have uniformly observed all those colours which I have mentioned when a current of nitric oxide is passed through strong nitric acid, even with a moderate degree of rapidity. The peculiar shades of colour are always present, and make their appearance in the order described, and I have never seen any deviation from that order.

Although I have already said sufficient to show what degree of confidence his assertions or experiments are entitled to, it will be satisfactory to the reader to see what Priestley, Kirwan, and Davy say of the action of nitric oxide on nitric acid, as they all paid much attention to this experiment.

Dr. Priestley made many experiments on this subject: the following are the changes of colour which he observed in one of them, of which he makes particular mention, where the acid was placed in a vessel containing nitric oxide. The experiment was made with " strong pale-yellow spirit of nitre."

" Presently after this process began" alluding to the absorption of the gas by the acid " the surface of the acid assumed a deep orange colour, and when twenty or thirty ounce measures of air (nitric oxide) were absorbed, it began to be sensibly green at the top; and this green kept descending lower and lower, till it reached the bottom of the phial. Towards the end of the process, the evaporation of the acid was perceived to be very great; and when I took it out, the quantity was found to have been diminished exactly one half; for there remained no more than the quantity of two pennyweights of water. Also it had become, by means of this process and the evaporation together, exceedingly weak, and was rather *blue* than green." *Experiments and Observations on different kinds of Air.* Vol. i. p. 384.

These were the observations of Priestley on this subject, made upwards o forty years ago. The following passage occurs in Kirwan's Essay on "Phlogiston and the Constitution of the Acids;" and there are others of a similar nature in different parts of the work.

> "*Red*, *yellow*, *green*, and *blue* nitrous acids, when those colours are intense, owe their origi to the absorption of nitrous air." p. 37.

In Sir Humphry Davy's Researches on Nitrous Oxide, &c. there is a full account of the appearances produced by passing nitrous gas (nitric oxide) through nitric acid; and he has even given tables on the subject, so that there can be n apology whatever for a reviewer, who, while he thus attacks another for ignorance, shows himself so grossly ignorant on the very point on which he found his charge. Sir H. Davy says,—

> "93 grains of the bright yellow acid of 1·5" prepared by transmitting nitric oxide through pale nitric acid, and which Sir H. Davy mentions assumed a straw colour, and then a pale yellow before the bright yellow colour appeared, "became, when 6 cubic inches of gas had been passed through it, orange coloured and fuming, whilst the undissolved gas increased in quantity so much as to render it impossible to confine it by the solution of potash. When 9 cubic inches had passed through, it became dark orange. It had gained in weight 2·75 grains and was become of specific gravity 1·48 nearly. Hence it was evident that much nitrous ga had passed through it undissolved. 25 cubic inches more of nitrous gas were now slowly sent through it: it first became of a light olive, then of a dark olive, then of a muddy green and lastly of a blue green." *Researches*, p. 26.

My words are "the acid speedily acquires a light straw colour, which deepens to a reddish brown, and passes through various shades of olive and green till it at last becomes almost blue." Such is the description which I have given of an experiment which any tyro in practical chemistry may easily perform and see all the appearances I have described in the course of a single minute when he operates on a small quantity of acid with a brisk current of gas; and it will be observed that my account corresponds with those of Priestley, Kirwan and Davy. But the reviewer states that "strong nitric acid never becomes at all either olive, green, or blue by absorbing nitric oxide."

After the above exposition, what must we think of a reviewer who has the cool effrontery to say, "What proves that Mr. Reid never performed the experiment, is, that when the colours are produced, they do not occur in the order stated by him," &c. What grounds could he have had for asserting that I had described the action "with extreme and most culpable inaccuracy?" It must not be forgotten, at the same time, that an account of these experiments performed long ago by the eminent chemists whom I have quoted, may be seen at the present moment in the Elementary Treatises on Chemistry of the present day.

> *Quotation IV. continued.*—" Nor is acid of any one degree of strength capable of exhibiting them all, whatever may be the quantity of nitric oxide passed into it; if, for example, pale acid of specific gravity 1.46 be used, the nitric oxide renders it for a moment yellowish, and then it becomes red, but never olive, green, nor blue at all." p. 454.

I have made the experiment frequently with pale acid of various specific gravities: with acid of the very specific gravity to which he alludes, and with acid both stronger and weaker than this. I have seen each of these kinds of acid become not only yellow and red, but also olive and green, and almost blue, as I have described, or, to use Sir H. Davy's expression, "blue-green." And any one who will take the trouble to perform this simple experiment, will see the same. I may be allowed to state also, that I have made the experiment with nitric oxide gas prepared in the usual manner, and with gas carefully dried previously over mercury. The gas also was sometimes passed through the acid, and on other occasions the acid introduced into the gas with every precaution.

From what I have now stated, and from the extracts from other anthors inserted above, it will be seen what degree of credit is to be attached to the reviewer's assertion, " nor is acid of any one degree of strength capable of exhibiting them all."

> *Quotation IV. continued*—" dilute a portion of this acid with an equal bulk of water, the nitric oxide then renders it green, but it assumes no other colour; mix three measures of the acid with four of water, and the mixture may be rendered blue, but neither yellow, olive, red, nor green."

These statements have nothing to do with the question. I have described, in the paragraph he quotes, the action of nitric oxide on strong nitric acid, and make no mention of the action of this gas on nitric acid diluted to the extent he specifies.

> *Quotation IV. continued*—"The mistakes committed by Mr. Reid in this part of the subject are the more remarkable, because in the very next page to that in which they occur, he shows that the production of the different colours does not depend upon the quantity of nitric oxide absorbed by strong acid, but upon that retained after various degrees of dilution; for he says if small quantities of water be added to the strong fuming acid 'it gradually loses its deep orange red colour, and passes through various shades of olive, green, and blue; and if a sufficient quantity of water be added, it becomes quite colourless.' "

I have already shown that what the reviewer calls " mistakes" are the plain expression of a fact, which any one can easily convince himself of by performing the experiments; but, in the words now quoted, there are also two misrepresentations, to which it is necessary for me to advert.

In the first place, who would imagine, after the words of the reviewer, that I have never stated, " that the production of the different colours does not depend upon the quantity of nitric oxide absorbed by the strong acid?" Who would think it possible that a reviewer, commencing with a profession of candour, could make such a statement, when, in the very sentence following the passage in which the action of the gas upon the acid is described, I have stated most explicitly that the change of colour does depend upon the quantity of nitric oxide absorbed. My words are, " The change of colour arises from the nitric oxide taking oxygen from the nitric acid, both being converted into nitrous acid, the one by losing, and the other by gaining oxygen, the depth of colour varying according to the quantity of oxide absorbed." *Elements of Practical Chemistry*, p. 61.

Secondly, In the paragraph in the next page to which he refers, I do not show, as he affirms, that the production of the different colours depends upon the quantity of nitric oxide retained after various degres of dilution. When I wrote that paragraph, I studiously avoided mentioning either the nature of the action that takes place, or of the gaseous matter evolved, not being satisfied with the account usually given of this experiment, and confined myself solely to a statement of the change of colour produced, till a future opportunity should enable me to examine the subject more minutely.

I subjoin the paragraph itself.

> " 146. If small quantities of water be added to the strong fuming acid (composed of nitric and nitrous acids) prepared by distilling nitre with two thirds of its weight of sulphuric acid, it gradually loses its deep orange red colour, and passes through various shades of olive, green and blue; and, if a sufficient quantity of water be added, it becomes quite colourless."

In the paragraph (number 145) in my book, immediately preceding that to which he latterly alludes, I describe the action of nitric oxide on colourless nitric acid; in this I state the change of colour that takes place, when " strong fuming acid," prepared from particular proportions of nitre and sulphuric acid is mixed with water.

In this single quotation, then, the reviewer has contrived to accumulat such a mass of unfounded charges and misrepresentations, that it would not b easy to find its parallel.

The reviewer has also been pleased to comment very freely on the manipulations in some of the processes which I have described, and here also I hav to point out numerous errors and misrepresentations.

Quotation V.—" To prepare nitric acid, Mr. Reid directs the use of equal weights of nitr and sulphuric acid, and recommends that the acid distilled should be condensed in receiver kept cold by a slender stream of water. Now, although in this operation the receive certainly becomes slightly warm, yet it is never rendered hot; and we know from direct experiment that within $\frac{1}{480}$ of the whole quantity of acid contained in the nitre may be procure without the aid of any artificial cooling whatever." p. 450.

The distillation may be conducted without any cooling of the receiver, an little acid be lost; but in all ordinary cases when this precaution is not adopted acid fumes, which are extremely disagreeable to the operator, and may prov very injurious when he is of a delicate constitution, are disengaged; and, fro the decomposition of the nitric acid, nitrous acid vapours are sometimes evolve in considerable quantity, as when a small proportion of sulphuric acid is used It is from having witnessed the effects of these, that I have recommended i all cases the application of cold to the receiver. This condenses much of th acid vapours that would otherwise be disengaged, and assists the absorption o the nitrous acid. If he had been better acquainted with an author whose view he afterwards charges me with neglecting, he would have seen the followin passage in his remarks on nitrous acid: " C'est cet acide qui se produit tout-à-coup par le mélange du deutoxide d'azote et de l'oxigene ou de l'air, et de l vient que le deutoxide d'azote est irrespirable, qu'il agit sur l'économie animal avec une énergie extraordinaire, et qu'introduit, même à très-petite dose, dan la poitrine, il y fait éprouver subitement un sentiment très-penible de constriction." *Thenard's Traite de Chimie*, t. ii. p. 185.

Quotation V. continued.—" When, however, it" (the cooling of the receiver) " is required the plan of placing the receiver in a vessel of cold water is much more simple and easy of execution, and requires much less attention than the mode of cooling recommended by Mr Reid."

I have frequently tried this mode of operating. Though apparently mor simple to an inexperienced operator, it is in reality much more difficult and less successful; and the mode which I have recommended was adopted in preference, because I found that the student conducted the operation more easil in this manner.

When the receiver is placed in a basin of water, if not fixed in its place an kept down by a weight, but allowed to find its own level, it will float unsteadil in the basin. Besides, the receiver is very apt to be broken when the lowe part is kept cold by the water, while warm vapours are collected in the uppe part; and also, the condensation is less speedily effected, the vapours not entering at first into the coldest part of the receiver. Now, in the mode recommended, which is the result of much experience, when the apparatus is adjusted, th receiver is perfectly steady; a constant supply of water at a uniform temperature is kept up; the whole of the receiver is nearly equally cool; and the war vapours, which naturally ascend, come in contact at once with the coolest par of the receiver, and are immediately condensed.

Quotation V. continued.—" In the figure representing the apparatus a flask is employed instead of a proper receiver; now this, on account of its form, must render the apparatus liabl to accident from unsteadiness ; a common bottle is much better, but a tubulated receiver used with it is greatly to be preferred."

A flask I have always found preferable as a receiver : all the receivers which I have seen, made here or in London, are of unequal thickness, and this is always the case with tubulated receivers, from the mass of glass required for the tubulure. A flask is never unsteady when the apparatus is arranged in the manner shown in the illustrative figure I have given, though a flask, placed as the reviewer recommends the receiver to be, is necessarily unsteady. A common bottle may be used by an experienced operator, or when the operation is conducted very slowly ; but every one knows that the thick glass of which common bottles are made, is extremely apt to be broken when surrounded by cold water at the very moment that warm vapours are entering into it ; and when chauffers are used, the students are seldom able at first to manage them, so as to maintain a steady heat. The distillation of nitric acid has been conducted in my class-room by different pupils upwards of two hundred times. When the operation was conducted as the reviewer recommends, the receivers were so frequently broken, that I varied the process in every possible way, till I at last adopted the method I have described, which I have no hesitation in preferring. Formerly, I have seen many receivers broken ; now this seldom happens. During the last seventeen distillations conducted by students commencing the study of chemistry, not one receiver was broken.

Quotation VI.—" 'The distillation of nitric acid,' says Mr. Reid, p. 54, 'may also be conducted in flasks with a long glass tube bent at one end in the manner shown in the figure, or the condensation of the acid may be effected almost entirely in the tube. This is a very convenient method of conducting the process, and is often preferred to distilling the nitric acid in a retort, though beginners find some difficulty in adjusting the tube. The nitre and sulphuric acid are first put into the flask, and a thin tube bent at an acute angle, about two inches from one extremity, and a very little less in diameter than the neck of the flask, is surrounded with some well worked clay, and put into it, a small quantity of plaster of Paris being placed over the luting to render it tight.' The reason for this apparatus being 'often preferred' Mr. Reid has not given ; nor can we discover any, unless that which is confessedly difficult to beginners is peculiarly easy to the initiated."

This operation is pointed out from the simple nature of the apparatus, and facility with which it may be formed. Practical Chemistry is now studied by naval and military officers, as well as by medical students and engineers. All these classes of students are particularly anxious to be enabled to draw largely upon their own resources, as they may frequently be in distant parts of the globe, where the usual facilities for operating cannot be procured : and any one who knows how to conduct such an operation, will be enabled to convert a flask with a small tube into a retort ; and there are, perhaps, few places where such vessels cannot be obtained. Most students of practical chemistry are taught the method of making tube apparatus, and many of them provide themselves with a set before they leave the country. Though beginners may sometimes find it troublesome to get the tube properly placed, if they attend to the directions given immediately after this statement to obviate the difficulty, nothing can be more simple or easy in execution than the adjustment of the apparatus. Were I to explain minutely the reason for each modification, the utility of which must be obvious to every one who reflects for a moment, the work would have been increased to a size incompatible with the object for which t was written.

Quotation VI. continued.—" We do, however, assert, without having tried, and without in-ending to attempt this process, that it is the most inconvenient which it is possible to devise."

Little comment is necessary on this remarkable passage. In all matters of practical experience, it is generally considered that a trial is necessary to enable one to judge of the advantages of any operation; but this writer, who professes a minute examination of the subject, who has told us that *fiat experimentum* is his motto, and holds out an intention of "acting candidly towards the author," in the very page in which he makes the latter declaration, condemns unhesitatingly, and in the most unqualified manner, a process described and recommended by one who, he must have seen, had tried it frequently, and who, he knew, had some experience at least in such matters; and which he declares he has never tried, and never intends to try.

The animus with which this sentence is written is sufficiently obvious, and has led the writer to urge the most absurd reasons, and to misrepresent my statement, as I shall show in the following paragraphs. It will be sufficient here to state, that in my classes for practical chemistry, which admit only a limited number of pupils, so that each may operate every day during the course, three hundred and thirty gentlemen who attended them during the last two years alone have witnessed the minutiæ of this process, and a great number of them have conducted it themselves; and frequently as I have seen it performed by beginners, I have not met with any reason for altering my opinion of its simplicity and convenience. I have nowhere stated that it is superior to the retort process; but I hold that it is one which, in many situations, it will be very convenient to know, and which it would be most improper to omit in a work, one great object of which is to train the student of chemistry to operate with as few and as simple materials as possible.

> *Quotation VI. continued.*—"a flask is much less proper than a retort, as will occur to any one who will for a moment compare the diameters of the descending tube, with the descending aperture of the neck of the latter: owing to the narrowness of the tube, much nitric acid which condenses necessarily runs back into the flask, and consequently the process is lengthened."

I have never affirmed that a flask is more proper than a retort; but nothing can be more erroneous or show a greater ignorance of the circumstances which modify these processes than the reason which the reviewer here gives for his preference to the retort. He tells us that, owing to the narrowness of the tube, much of the acid condenses and runs back into the flask, whereas it must be obvious to every one who reflects on the nature of the apparatus, that it must be less favourable to the condensation of the vapours before they have passed the angle than the retort. In the part of the tube before the angle there must be less condensation than in the corresponding part of the retort, because the tube has the neck of the flask between it and the cold air, and thus the vapours have a double protection from the cold; and also in the upper part of the flask, which is narrow and concave, there must be much less condensation than in the corresponding part of the retort, which is wide and convex, and exposes a very large surface to the action of the cold air.

> *Quotation VI. continued.*—"It must be difficult, not only to the beginner, but also to the veteran, to fasten the glass tube into the flask with clay; and to render it secure, two lutes are recommended, when in common cases one is sufficient; a thin tube when bent is extremely apt to break at the angle, more especially when it is to be fastened into two flasks, one of which is unsteadily supported on a ring, and the other equally so on its side."

The writer appears to be unacquainted with the most common and simple processes of manipulation, otherwise he would not tell us that it must be difficult to fasten a glass tube into a flask with clay. Students can never find any difficulty in arranging the tube with the flask when they attend to the directions I have given. But of course the simplest operation will appear complicated to those who are deficient in practical knowledge.

He next tells us that a thin tube when bent is very apt to break at the angle!

In the next line we have a misrepresentation of the process as I have described it. He says "more especially when it," (the tube) "has to be fastened into two flasks," giving this as another cause why the tube would likely give way at the angle. I do *not* say that the tube is to be fastened into *two* flasks; it has to be fastened only into one, and rest loosely in the other, and every one will see how materially this diminishes the chance of its being broken. My words are, after directing one extremity of the tube to be fastened into the flask containing the materials with clay and plaster of Paris, "The acid is condensed in a receiver, into which the other extremity of the tube is introduced." He has quoted the whole of the paragraph except these concluding words; thus stating that I have directed to be done what I have not directed to be done, evidently for the purpose of giving greater weight to his objections to the operation, and has carefully omitted in the quotation the two concluding lines which would completely have disproved his statement.

The next line contains another gross misrepresentation. He says, with the view of accumulating objections to the operation, "one of which" (the flasks) "is unsteadily supported on a ring." It is not unsteadily supported on *a* ring; in the figure illustrating the process the flask is placed on *one* ring, and another and smaller ring is placed above in such a manner, round the neck of the flask, that it cannot be upset, except by a force which would upset the retort-stand supporting it. So many are the mistakes which a writer, apparently unacquainted with practical operations, makes in commenting on them. So many are the misrepresentations of the author's description which a critic, determined to cavil, has the hardihood to introduce.

In the following paragraph we have many instances of the reviewer's misrepresentations and efforts to distort my statement:—

> *Quotation VII.*—"After having mentioned the weights of nitre and sulphuric acid proper to be employed, and adverted to the circumstances attending the use of one atom each of the acid and salt, we did not expect that our author would have given further instructions on the subject; but to our surprise the following directions occur at p. 56: 'in conducting this process on the small scale, three ounces (water measure) of sulphuric acid to eight ounces of weight by nitre will be found convenient proportions.'"

I do *not* give further instructions on the subject. The process mentioned in the quotation from my book in the two last lines of the preceding extract, *is a different process* from that mentioned in the first line of this extract from the review. I have in the beginning of the chapter given the proportions for preparing nitric acid, and obtaining all the acid from the nitre; (or, as he says, the weights of nitre and sulphuric acid proper to be employed); I have described the usual method of conducting the distillation, and another mode which may be occasionally resorted to; and I conclude by explaining the action that takes place. This description, then, is complete; it refers to one process: and I add nothing farther regarding it, that is, nothing farther on the subject on which the reviewer incorrectly states that I give "further instructions."

In a new paragraph, with a new number, I mention that a different proportion of materials is sometimes employed; I explain the action that takes place in this case; I mention that we do not obtain the whole of the acid which exists in the nitre; and that the product is different from that obtained by the former process. I then proceed to describe the method (the one at which he is surprised) of performing this process on a small scale, by which we obtain an acid composed as I have described in the preceding paragraph. This the reviewer calls giving "further instructions," and states it as if, after having given sufficient directions for performing a process, I had inserted about the *same process*, additional instructions, differing from the former, and tending only to confound the student or junior experimentalist; whereas the two processes are brought out as quite distinct, and the directions to which he refers, apply to an easy mode of obtaining the proper proportion of acid in performing this process. One would think from the reviewer's statement that only one process had been previously described; he omits mentioning the process in which the proportions are different (fully described by me in the paragraph preceding that which he criticises), while he objects to the proportions I have directed for the small scale of this operation, because, as he tells us, they are at variance with the proportions given for a different operation. These proportions and additional directions regarding the latter process were given for the purpose of enabling the student who wished to make a small quantity of this acid (sufficient for most purposes for which a junior experimentalist would require it), to *measure* the sulphuric acid that he might not have the trouble of weighing it. Here, then, with the view of adding to the charge, he has made it appear that I have confounded two processes, which, in my book, are stated and seen to be different, as clearly as the section on nitric acid and that on atmospheric air.

> *Quotation VII. continued.*—" The quantities which had been previously mentioned, we supposed were intended for operations on the small scale, since flasks are the vessels in which they are directed to be conducted."

Whatever suppositions the reviewer may choose to make, my description admits of no such interpretation, and it is impossible that any one could put such a construction on my words. The quantities previously mentioned were distinctly directed to be used in the *retort process*, and there is no mention made there of a small scale process; the retort process may be used either for the large or small scale, as I do not mention *quantities*, but *proportions;* I direct "bruised nitrate of potash to be exposed to heat, with an equal weight of sulphuric acid," and immediately after this, in the same paragraph, give directions for conducting this process and illustrative figures.

The reviewer also tells us, that flasks are the vessels in which the operation is directed to be conducted. This is not the case: there is no operation whatever *directed* to be conducted in flasks: I have merely described this as a variety of the retort process—as a mode of conducting the distillation which is occasionally found very convenient.

> *Quotation VII. continued.*—" An ounce water measure may, we conceive, have several meanings; first, the measure of an avoirdupois ounce of water; secondly, the fluid ounce used in the London Pharmacopoeia; and, thirdly, the measure of a troy ounce of water: so that the student has the chance of using a measure equal to 480, 454·5 or 437·5 grains of water."

This is another statement for which there is not the slightest ground. An ounce (water measure) mentioned in my book can only have one meaning; an ounce by fluid measure according to the table of fluid measures given at the end of my work,—according to the only fluid measure in use. Were it possible that any person could be in doubt about the meaning of the word, what would he do? Would he, as the reviewer suggests he might, take a measure equal to the bulk of a quantity of water, weighing an avoirdupois ounce (437·5 grains)? Certainly not; because there happens to be no such measure in use; and because, if there were, it is not mentioned in the tables in the book where the word occurs. But, would he take the measure of a troy ounce weight of water (480 grains)? Certainly not; for the very same reasons for which he would not take the avoirdupois ounce. How inconceivably absurd it is to suppose that any person, on being told to take a *measure*, would go and look for a *weight*, and fix his measure by the quantity of water equal to this weight; that he would make a standard of his own, without asking for the measure always used for chemical purposes in measuring water and other liquids. What then would he do? He would take the only liquid ounce measure in use, and which is fixed to contain 455·6 grains of water. Should he not know previously about this fluid measure, he would naturally turn to the tables of measures, where he would see only one table of fluid measures descending to ounces, in which the value in grains of a gallon, a pint, an ounce, a drachm, and a minim is shown in a tabular view. The only measure then which the student could take is the one he would naturally ask for, the apothecaries' measure, (the measure of the London pharmacopœia.)

In looking at the tables he would also observe, that this measure corresponds with another table I have given, "showing the weight in grains of various measures of different fluids," in which cold water and sulphuric acid are mentioned; so that it is absolutely impossible that any one could be in doubt.

Quotation VII. continued.—"Nor is this all; he is directed to take eight ounces of nitre, and 'by weight' for fear he should measure them; but as to whether the ounces are to be avoirdupois or troy weight, he is totally without a guide."

The student is not without a guide. It is well known that the troy weight (now the imperial standard weight) is always used in such operations; but if he should not know this, he would find in the apothecaries' weight an ounce exactly of the same value as that in the troy weight; tables of both of which are given at the end of the book. Independently of this, he could be in no doubt; he would recollect that in the paragraph immediately preceding, the proportions were stated to be two to three, and this would settle the point at once, as, if there were any doubt about the measure he had only to weigh the materials in these simple proportions. The reviewer, however, has cautiously omitted mentioning this circumstance.

Quotation VII. continued.—"As, however, the quantities, whatever they may be, are specially directed for the 'small scale,' the student may suppose that they are to be as different as he can make them by the laxity of the rule, from the proportions previously assigned."

The operation here described is not the small scale of the process with "the proportions previously assigned:" it is, as I have already mentioned, a different process. But, supposing that the processes were the same, no one whatever could imagine that proportions for making a small quantity of acid should be "as different as he can make them by the laxity of the rule," from the proportions for making a large quantity: this is such an unnatural supposition, that no one, but one anxious to misrepresent, or totally ignorant of practical chemistry, I would almost say of the simple rule of proportion, could hazard such an assertion. I have already sufficiently shown that there is no laxity of the rule; that precise and minute directions are given.

Quotation VII. continued.—" In this case he may take the avoirdupois ounce of water for the measure of the acid, and the troy ounce in weighing the nitre; and then the weight of the acid will be to that of the nitre about as 2 to 3·16, which are indeed very nearly the quantities given in the Edinburgh Pharmacopoeia, and proportions less in unison with the doctrine of equivalents could hardly be adopted." p. 452.

I have already shown that the student could take no proportion but those intended; and, in the way I have given them, they are 2 of acid to 3·047 of nitre, almost exactly the proportion of the present Edinburgh and former Dublin Pharmacopœia, and as near as it was possible to give in round numbers.

In this latter quotation, he says, " proportions less in unison with the doctrine of equivalents could hardly be adopted." This is a method of attacking me which this candid reviewer frequently adopts. In many parts of the review, he asserts, that such a view or statement is incorrect, as if in refutation of my account of it, while I have never in any part made the statement which he pretends to refute. In this case, not only have I never said that this proportion of materials was in unison with the doctrine of equivalents; but in the paragraph preceding that which he has chosen to criticise, I have particularly mentioned the action that takes place, shown that we do not obtain all the acid in the nitre, and the reason why nitrous acid is present; while, in the paragraph preceding that, I have given a minute account, illustrated by a diagram, of the action that takes place when the proper proportion for obtaining nitric acid is used—the proportion in unison with equivalent numbers.

Quotation VIII.—" We think we can also defy Mr. Reid to point out a single process in which the coloured acid is to be preferred to the pale." p. 452.

After this tone of defiance and self-confidence, the reader who may not have paid particular attention to chemistry, will be surprised to hear that even in Priestley's time facts were well known, which would lead any one in particular cases to prefer the coloured acid to the pale. Thus, in the preparation of nitric oxide gas, though the pale acid in all ordinary cases is in common use, still this gas may be procured more speedily from the coloured acid, and I employ it, and prefer it on this very account, when I wish to procure speedily a current of this gas. The following are Dr. Priestley's words,—" It is very remarkable, that when copper is dissolved in pale spirit of nitre, even diluted with much water, though the solution is evidently the more rapid at the first, the produce of air is very trifling for a considerable time, and the quantity collected increases very gradually; whereas, when the orange coloured acid is employed, in the same diluted state, the nitrous air," (nitric oxide) " is collected immediately; and the production is the most copious at first."—*Experiments and Observations on different kinds of Air,* 1790, vol. i. p. 406.

Were it necessary, I might quote other authors, who have in particular processes preferred the coloured to the pale acid, and there are other processes where, from experiments made to ascertain which ought to be preferred, the strong mixed acid was always found to be preferable.

In the very same paragraph also, in the sentence immediately preceding the words we have quoted, the reviewer states.—" When nitric oxide gas is passed into colourless nitric acid, it becomes red, and its density is considerably increased." Page 452. In reference to this, we have only to remark, that Sir H. Davy made many experiments on the specific gravity of the coloured acid procured by transmitting nitric oxide through pale nitric acid, the particulars of which are described in his Researches, where, referring to one of them, he states, at page 25, "learning from it, that nitric acid was diminished in specific gravity by combination with nitrous gas," &c.

Quotation IX.—" At page 60 our author states that 'nitric acid (by which the common liquid nitric acid is always understood, composed of one equivalent of real acid and two of water') possesses certain properties, all of which it is not requisite to quote; but he tells us that 'it is distinguished from all other acids by the facility with which it affords oxygen to metals and combustible bodies, most of which decompose it with great rapidity.' This the reader will remember is stated of acid of sp. gr. 1·5; and yet in direct contradiction to this, and only six lines further on in the same page, we are informed that nitric acid of sp. gr. 1·48 'is scarcely affected by the metals at ordinary temperatures,' requiring the addition of a 'small quantity of water' to cause its decomposition. Here then occurs another instance of that want of care of which we have given so many;—nitric acid of sp. gr. 1·5, containing of course less water than that of 1·48, is decomposed by metals 'with great rapidity;' while that of 1·48, which contains more water than that of 1.50 'is scarcely affected by the metals at ordinary temperatures, without adding a small quantity of water.'" p. 453.

Here I must in the first place show the manner in which he has misrepresented the text, so as to make a statement of mine appear a contradiction, though any one on reading my book would at once perceive it to be brought out as a peculiarity in the history of the acid. The passage in the above quotation to which I refer is—" This the reader will remember is stated of acid of sp. gr. 1·5; and yet, in direct contradiction to this, and only six lines further on in the same page, we are informed that acid of sp. gr. 1·48," &c. Now, any one trusting merely to the reviewer's account would naturally believe that this was an inconsistency on my part, arising from ignorance or inattention: but let us see how it is stated in the book. After having described the properties of nitric acid of specific gravity 1·5, as the reviewer mentions, I state, "But when it is of a particular specific gravity (1·48) it is scarcely affected by the metals at ordinary temperatures; on adding a small quantity of water, however, a decomposition immediately begins, the metal often taking oxygen both from the acid and the water." Elements, p. 60. Thus it will be evident to every one, that instead of being stated so as to imply a *contradiction* to the previous statement, it is most distinctly and unequivocally brought out as an *exception;* and in such a manner as to draw the attention of the reader to the peculiarity. It is impossible that any one could imagine it to be a contradiction, when the sentence begins with " But," and the words "*particular* specific gravity" follow in the same line. The reviewer, however, has suppressed the words—" But when it is of a particular specific gravity," though they *immediately precede and form part of the very sentence which he partially quotes;* and it will be seen how carefully he has selected particular lines only, still including them within inverted commas, that they may appear to give a fair view of my statement. Had these words, which he has omitted, been included in his quotation, they would at once have refuted the charge of contradiction which he has founded on the garbled quotations.

In his eagerness here to point out a contradiction where none really existed, he has again entrapped himself in his own snare, and has betrayed his ignorance of the peculiar constitution of nitric acid. It is quite obvious that the reviewer has here concluded that nitric acid must be analogous to acids in general, and that there is nothing peculiar in acid of specific gravity 1·48.

The statement to which he objects was founded partly on a peculiar circumstance first pointed out by Proust, of which it is evident that the reviewer was ignorant, and partly on what I myself had frequently observed on exposing nitric acid of various densities to the action of the metals. Proust, upwards of thirty years ago, published in the Journal de Physique, vol. xlix, p. 59, an account of some singular phenomena connected with nitric acid; from which I make the following extracts:

"On voit par-là que cet acide se comporte, dans sa concentration, d'une manière bien opposée à nos idées, et qui est l'inverse de celle de la plupart des autres acides."——

"On peut donner, dans les leçons, une idée de la force d'attraction qu'exerce sur lui-même un acid concentré par l'expérience suivante; c'est de verser de l'acide à 1·48 sur de l'étain pulvérisé par la methode connue dans les pharmacies, et il n'en résulte pas plus d'effet qu'avec du sable; mais il n'en est pourtant pas de même avec le zinc."

Mr. Dalton, in the second part of his New System of Chemical Philosophy, published 1820, states that nitric acid, with a certain quantity of water, "is possessed of striking peculiarities." In the same page, he alludes to Proust's experiment with tin, which I have just quoted; and, in the following page, he adds, "The fact I find as Proust states it."

But the reviewer might have seen, in other works besides these, many statements that would have directed his attention to the subject, should this peculiarity not have occurred to him in the course of his own experience.

Dr. Henry makes the following observation, in allusion to the action of the metals upon this acid.

"The hydro-nitric acid is also decomposed by metals; as iron, tin, zinc, copper, &c. with different phenomena according to the affinity of each metal for oxygen. This may be seen by pouring some strong nitric acid on iron filings or powdered tin. The acid must be of greater density than 1·48, otherwise it will not produce the effect." And it is a well known fact, that acid of a less specific gravity than 1·48 is acted on violently by many metals: this, however, our learned reviewer at once declares to be a contradiction. Dr. Ure also mentions the same fact in respect to tin.

With regard to the boiling point of nitric acid, there is a peculiarity which is now well known to chemists, and which might have reminded the reviewer of the necessity of acting consistently with his motto, and he would not then have been led away by what he imagines an inconsistent statement, the inconsistency in which he himself makes. The other fact to which I now allude is, that very strong nitric acid is rendered weaker by boiling, while weak acid is rendered stronger by boiling, and acid of an intermediate strength distills without any change.

The following experiments confirm the accounts which have been given of the peculiarities of nitric acid at different densities.

1. Nitric acid, of specific gravity between 1·50 and 1·51 was used. Four glasses were taken, and an equal portion of acid poured into each. A few drops of water were added to one of these, twice as much water to another, and four times as much to a third: the fourth was not diluted. Pounded antimony was then thrown into each till it rose to the top of the liquid, when it was found that an effervescence took place in the strong acid, and in the acid to which most water had been added, while the antimony had comparatively but a feeble action on the acid to which the smaller proportions of water had been added.

2. I took acid of the specific gravity 1·48, and added a similar quantity of pounded antimony to it, and it was not nearly so much acted upon as the strongest and weakest acids used in the preceding experiment. The difference was very marked.

The reviewer here therefore compels me to make the same charges against him as in other instances—ignorance and misrepresentation. The misrepresentation here is perhaps as gross as in any other part; the ignorance is indeed extraordinary in a critic.

Quotation X.—" Let us then suppose that the acid is to be diluted; yet when it is decomposed by metals 'all the oxygen, however, is not withdrawn from the nitrogen, nitric oxide, and nitrous acid, being generally disengaged during the action that takes place.' These statements are, we apprehend, not quite correct: we admit that nitric oxide is usually evolved, and sometimes nitrous oxide is mixed with it; but the nature of the gases depends upon the strength of the acid and the nature of the metal; for zinc and very dilute nitric acid, give nitrous oxide nearly pure and unmixed with nitric oxide; and we much doubt whether nitrous acid is in any case whatever evolved. Has not Mr. Reid mistaken the production of nitrous acid, by the action of nitric oxide upon the oxygen of the atmosphere, for its direct evolution by the action of the metal?" P. 453.

Here, as usual, his anxiety to find me in the wrong, has led himself into error. When he doubted whether nitrous acid was in any case whatever evolved, one would naturally have concluded that he would have recollected his motto, *(fiat experimentum,)* and made an experiment to ascertain whether he was correct or not, before questioning the accuracy of the statement I had made, and thus saved himself the trouble of inventing what he appears to think a plausible mode of accounting for the mistake which he supposes I had committed. I know, from experiment, that nitrous acid is evolved by different metals when they act on colourless nitric acid in circumstances where no fallacy can arise from the evolution of nitric oxide, and its action on atmospheric air; and any one may easily satisfy himself of this by causing iron, nickel, copper, tin, zinc, or bismuth to act on the acid, af- er adding a little water, in vessels filled with carbonic acid gas, so as o exclude the action of the air. The experiment may be varied many ifferent ways, which will naturally suggest themselves to every practical hemist, and the result so speedily obtained, that if the reviewer should ever ave been in the practice of operating, it must certainly appear surprising that e did not himself try the experiment before hazarding an assertion which very tyro in practical chemistry can so easily disprove. Nor is it easy to onceive how any one at all familiar with chemical operations could not have ad this fact forced again and again upon his attention, though he should only ave performed a very limited number of experiments.

But this evolution of nitrous acid during the action of metals on nitric acid as long been familiar to chemists. In the second volume of Dr. Black's lec- res, edited by Professor Robison, it is frequently mentioned. At page 624, describing the action of tin upon nitric acid, his words are, "The acid is olently changed and decompounded; part is changed into nitrous acid; part to nitrous air; and a considerable part even into azotic gas."

Further, the reviewer states, as a piece of information, that the nature of the ses evolved depends upon the strength of the acid and the nature of the etal. But he has not stated what is perhaps equally well known, and throws uch light on the present subject,—that there are metals which are acted on nitric acid either with a copious evolution of gaseous products, or without e disengagement of a single particle according to the relative proportions in hich they are mixed together. This may be easily seen by the following mple experiments, the latter of which also proves most unequivocally the evo- ion of nitrous acid gas when mercury acts on nitric acid.

1. Take a common test tube, fill it half full of colourless nitric acid, and pour o it ten or twenty grains of mercury. In a few minutes, the mercury is mpletely dissolved, a considerable agitation is observed in the lower part of e liquid, and much gaseous matter is produced; but none of it escapes, being sorbed by the superincumbent liquid before it reaches the surface.

2. Take a stout mercurial pneumatic jar from one to two inches in diameter, and from one to ten inches deep; fill it with mercury and invert it over the shelf of the mercurial trough. Then introduce into it about a drachm of the colourless acid used in the preceding experiment, taking care to avoid the introduction of any air, and using for this purpose any of those numerous modifications of the dropping tube that are now so much employed;—a common glass blowpipe with the point bent a little upwards does very well. In about a minute in general, after the acid has risen to the top of the mercury in the jar, and when it is of the usual specific gravity, red fumes begin to appear, and the mercury slowly descends, leaving the jar quite full of the ruddy vapours, and affording another instance of their production when metals act upon nitric acid, totally independent of the action of the air.

In his remarks on the test proposed by Liebig for detecting nitric acid (which, however, it has been proved very lately, and since my work was published, cannot be depended upon,* the reviewer states:

> *Quotation XI.*—" We may also observe that Liebig's process is not intended for the detection of uncombined nitric acid, as Mr. Reid supposes, but of a nitrate; and the use of the sulphuric acid is to separate the nitric acid from its base, that it may act upon the indigo." P. 455.

Here the reviewer has entirely mistaken the nature of Liebig's process. He says that it is not intended for the detection of uncombined nitric acid; in the next line he tells us that the object of adding the sulphuric acid is to free the nitric acid from its combination—to render it " uncombined," that it may ac upon the indigo; and does not appear to be capable of drawing the very obvious inference that nitric acid which has not to be detached from a base, being already free in the liquid, would produce the same effect.

It is true that this test was more generally employed to detect a nitrate, because this is more frequently an object with chemists than the detection of nitri acid; but it is only by ascertaining the presence of the acid that it is of any us in detecting the nitrate; and hence it was used as a test for nitric acid.

Those who have merely a general knowledge of chemistry, may perhaps imagine that nitric acid at the moment of its disengagement from a base, may b more disposed to act upon the indigo, than if it had been previously uncombin ed. This, however, as every chemist knows, is not the case; and this test Liebig's, as I know from experiment, is as certain in its indications when use with a liquid containing uncombined nitric acid, as in cases where the nitr acid is detached from a base by the action of sulphuric acid.

I have hitherto spoken only of the purposes to which Liebig's process *mu* be applied (that is, upon the supposition that it is a good test, which was belie ed when my book was written); and of the justice of the reviewer's assertio as we could judge of it from the nature of the action: but he tells us that th test was not *intended* for the detection of uncombined nitric acid. Let us e quire then what Liebig himself says on the subject. At page 80 of the 35 volume of the Annales de Chimie et Physique, in a " Memoire sur la Substan amère produite par l'action de l'acide nitrique sur l'indigo, la soie et aloe Liebig has the following words:

" Pour pouvoir découvrir l'acide nitrique dans la solution, je me suis ser d'une méthode qui me paraît nouvelle, et à l'aide de laquelle on peut démontr la présence de $\frac{1}{400}$ d'acide nitrique.

" A cet effet, on mêle la liquide à examiner avec autant d'indigo qu'il

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* See a paper on this subject by Dr. O'Shaughnessy, in the Lancet of May 29, 1830.

nécessaire pour le colorer en bleu distinct ; on ajoute quelques gouttes d'acide furique concentré, et on chauffe à l'ébullition. Si le liquide contenait un rate, il sera décoloré, ou si sa quantité était moindre, la couleur bleue pas- ait au jaune. En ajoutant au liquide un peu de muriate de soude avant de voir chauffé, ou découvrira encore facilement $\frac{1}{500}$ d'acide nitrique."

Thus it will be seen that Liebig's test was intended by himself for nitric d, as well as to ascertain the presence of a nitrate : it had always been con- ered (till lately when its fallacy was pointed out) as a test for nitric acid : and, hat is rather curious, in a former number of this very journal which now says at the test is not intended for the detection of uncombined nitric acid, the fol- wing account is given, exactly in the manner here represented.

" TEST OF NITRIC ACID.

" Dr. Liebig proposes the following process for detecting the presence of tric acid : the fluid to be examined is to be mixed with as much sulphate of digo as will give it a distinct blue colour, and after adding a few drops of lphuric acid, the mixture is to be boiled.

" If the fluid contains a nitrate, the blue colour will be discharged, or only ndered yellow if the quantity of nitrate is very minute. Dr. Liebig states, hat by this process nitric acid may be detected when there is not more than four-hundredth of it present ; by adding a little common salt to the fluid efore applying the heat, even a five-hundredth of nitric acid may be readily etected.—*Ann. de Chimie*, xxxv. 80." Philosophical Magazine, New Series, ol. ii. p. 388.

Quotation XII.—" We have now arrived at the concluding paragraph of the chapter on nitric cid ; it is as follows : ' Oxygenated nitric acid may be prepared much in the same manner s oxygenated water, the deutoxide of barium being dissolved in diluted nitric acid, the bary- es removed by sulphuric acid, and the liquid that remains strengthened by evaporation in the xhausted receiver of the air pump. It presents the same general phenomena with many of he metals and metallic oxides, &c. as oxygenated water, but has not hitherto been applied to ny use.' In the year 1818 (Annals of Philosophy, vol. xv. p. 378), M. Thenard announced o the Academy of Sciences, that ' by carefully dissolving superoxidized barytes in nitric acid, nd precipitating the barytes from it by sulphuric acid, the excess of oxygen remains united with the former acid, which by this means becomes oxygenized nitric acid.' How soon M. Thenard discovered that he had mistaken the nature of the compound formed, we do not know ; but in the fourth edition of his *Traité de Chimie*, published in 1824, he says, (vol. ii. p. 83), ' Puisque les acides donnent plus de stabilitié à l'eau oxigénée, c'est sans doute en se combinant avec le peroxide d'hydrogène : du moins, dans l'état actuel de la chimie, la composition de ce peroxide rend tout autre hypothèse invraisemblable. A la vérité, cette opinion n'est pas celle que j'avais adoptée d'abord ; j'avais pensé que l'oxigène se combinait avec les acides, et qu'il en résultait un grand nombre de nouveaux acides oxigénées.' As the result of further experiments, he concludes : ' Je compris et je reconnus bientôt que ce qui m'avait paru être des acides oxigénées n'etait que l'eau oxigénée et acidifiée.' It is clear, therefore, even in the opinion of Thenard himself, that no such compound as oxygenated nitric acid exists : and it is singular that Mr. Reid should have stated that as a fact, which had been six years at least exploded by its promulgator ; more especially as there is not, that we know of, one work on the subject of chemistry written during that period, which contains any notice of the existence of such an acid." P. 455.

This is another notable specimen of the anxiety of the reviewer to catch at any pretext for cavilling and misconstruing my words. In the 87th page of the volume to which he refers, and at the top of the third page which he quotes, Thenard makes the following observation, which the reviewer would have seen had he even taken the trouble of turning the leaf next the page he quoted, for the words that justify my using the expression could not then fail to have attracted his attention, as they are printed in Italics.

" Dans tout se qui suit, je désignerai chaque liqueur oxigénée par le nom d
" l'acide qu'elle contiendra ; je dirai donc *liqueur oxigénée nitrique, liqueu*
" *oxigénée hydro-chlorique*, etc., et par conséquent je les comprendrai toute
" sous le nom de *liqueurs oxigénées acides.*"

As I have expressed no opinion whatever as to the precise nature of the com pound, all the remarks of the reviewer refer solely to the name which I hav given it, and from this all his conclusions are derived. How then are the term that Thenard adopts—*liqueurs oxigénées acides, liqueur oxigénée nitrique, &c.* to be translated. These expressions, besides the awkwardness of their li teral translation, convey no precise idea of the constitution of the compound nor have they to my knowledge ever been literally translated in this country Dr. Turner gives these compounds no name whatever ; and Dr. Ure, in the fourt edition of his valuable dictionary published a few days ago, employs the origina terms which I have still retained. It would, indeed, have been singular had I, fron ignorance of his views, stated any thing in opposition to Thenard, as I happene to be engaged in chemical pursuits at Paris, shortly after the edition he allude to was published, and paid particular attention to it at that time. This matte is altogether so insignificant, that I should not have thought it worthy of notice but it displays the captious spirit of the reviewer.

From what I have stated above, the reader will perceive how far the reviewer is competent to judge of works on Chemistry : he will be able to appreciate the nature of the zeal and the extent of knowledge which have combined to produce this extraordinary review.—Who could imagine that all these misrepresentations, garbled quotations, inconsequent reasonings, and experimental blunders are crowded into a single review ; thus forming a critique that is perhaps unparalleled in the history of science.

To those who are conversant with Chemistry this reply may appear uncalled for ; many, however, who are but generally acquainted with the science, might otherwise take mere audacity of assertion for proof, particularly as the writer, in the confidence of his strength, comes forward with professions of a minute examination.

I have now done with this reviewer. In bringing forward this reply, I have not the presumption to think that any thing which concerns me personally could for a moment claim the public attention, but every man's character is of value to himself. That this work, particularly a first edition, should not be open to criticism, I am far from having the vanity to suppose, and corrections, suggested with any thing like candour, will be acknowledged as a favour, and met in that spirit in which they are given. I trust I know what is due to an open and manly opponent, as well as to the sober character of scientific investigations. In the present instance, I am not aware that I have been over-sensitive, considering the nature of the charges which I have felt myself called upon to answer.